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Published in:
EPRINTS-BOOK-TITLE

IMPORTANT NOTE: You are advised to consult the publisher's version (publisher's PDF) if you wish to cite from it. Please check the document version below.

Document Version
Publisher's PDF, also known as Version of record

Publication date:
2005

[Link to publication in University of Groningen/UMCG research database](#)

Citation for published version (APA):

Kuipers, T. A. F. (2005). On bridging philosophy and sociology of science. Reply to Jesús Zamora Bonilla. In *EPRINTS-BOOK-TITLE* University of Groningen.

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Theo A. F. Kuipers

ON BRIDGING PHILOSOPHY AND SOCIOLOGY OF SCIENCE
REPLY TO JESÚS ZAMORA BONILLA

There is a difficult relationship between present-day sociologists of science and social epistemologists, on the one hand, and “neo-classical” philosophers of science, on the other. Both parties have difficulty in taking each other seriously. Hope should be derived from those scholars who seriously try to build bridges. Of course, bridge builders have to start from somewhere and the most promising constructors with a philosophy of science background are in my view Alvin Goldman (1999), Ilkka Niiniluoto (1999), and, last but not least, Jesús Zamora Bonilla (2000). In the latter’s contribution to this volume Zamora Bonilla continues his very specific project of clearly specifying a kind of research agenda for studying bridge issues, in critical response to Ilkka Kieseppä’s reservations about a methodological role of the theory of verisimilitude and David Resnik’s arguments against the explanation of scientific method by appeal to scientific aims. Some of his main points are the following. (1) Gaining “recognition” is the dominant personal motivation of scientists, followed by trying to serve epistemic values. (2) Epistemic values can be served by methodological norms. (3) The norms have to be chosen under a “veil of ignorance” regarding the fate of the theories that will be proposed by certain scientists and hence the recognition they will get from them. (4) Hence, the most common norms in practice will best serve the relevant epistemic values. (5) Conversely, an adequate epistemic theory should enable us to justify these norms. (6) The HD method is very popular among scientists and is favorable for truth approximation, at least when both are explicated along the lines of ICR or along related lines, as presented by Zamora Bonilla. (7) The theory of truth approximation even justifies the popularity of the HD method.

Zamora Bonilla concludes with:

One possible challenge for those epistemologists who defend other kinds of cognitive utilities would be to justify that these other preferences just as well explain as the theory of verisimilitude the methodological norms *actually* adopted by scientists. Sociologists of

In: R. Festa, A. Aliseda and J. Peijnenburg (eds.), *Confirmation, Empirical Progress, and Truth Approximation* (Poznań Studies in the Philosophy of the Sciences and the Humanities, vol. 83), pp. 370-372. Amsterdam/New York, NY: Rodopi, 2005.

science should also try to offer alternative explanations of the extreme popularity of the hypothetico-deductive method (p. 366).

I would like to accept the first challenge and make the second somewhat more precise. Before doing so, though, I quote a statement from SiS (pp. 349-50).

To be sure, scientists not only aim at cognitive goals like empirical success or even the truth of their theories, but they also have social aims like recognition and power, and hence means to reach such aims. And although these goals frequently strengthen each other, [the existence of] such convergences by no means implies that the conscious pursuit of these social goals is good for science.

By arguing that epistemic values are subordinate to recognition and methodological norms subordinate to epistemic values, the latter on the basis of a veil of ignorance regarding the ultimately resultant recognition, Zamora Bonilla greatly relativized the possible negative effects of the conscious pursuit of recognition for the pursuit of epistemic values such as empirical success and truth.

To What Extent Are Instrumentalist Epistemic Values Sufficient?

A dominant line of argumentation in ICR is that the instrumentalist methodology, that is, HD evaluation of theories, is functional for truth approximation. Hence, that methodology serves the sophisticated realist cognitive values, and hence, conversely, these values can explain and justify the popularity of this methodology, to wit comparative HD evaluation. So far I agree with Zamora Bonilla. However, I would also claim that this methodology serves instrumentalist epistemic values, notably empirical success, at least as well. At first sight, Zamora Bonilla seems to disagree, but this might be mere appearance. The reason is that his own explication of truth approximation (see Zamora Bonilla 2000, and references therein) is essentially of an epistemic nature. Like Niiniluoto's (1987) notion of "estimated truthlikeness," it is not an objective notion. However, unlike Niiniluoto's notion, that of Zamora Bonilla is not based on an objective one. On the other hand, like my objective explication, and in contrast to Niiniluoto's explication, Bonilla's explication straightforwardly supports HD evaluation. Hence, the question is whether Bonilla's explication goes further than instrumentalist epistemic purposes. If so, my claim would be that even his explication is more than strictly necessary for explaining and justifying HD evaluation. However, this is not the occasion to investigate this in detail.

For the moment the interesting question remains whether there are other reasons to favor the (constructive) realist epistemology relative to the instrumentalist one. In ICR I have given two such reasons, one of a long-term and one of a short-term nature. Only the realist can make sense of the long-term dynamics in science, practiced by instrumentalists and realists, in which

theoretical terms become observation terms, viz., by accepting the relevant theories as the (strongest relevant) truth. This general outlook enables the realist to relativize for the short term a counterexample to a new theory that is an observational success of a competing theory by pointing out the possibility that the latter may be accidental (ICR, p. 237, p. 318) or, to use my favorite new term, that it may be “a lucky hit.” In sum, although both epistemologies can explain and justify the popularity of the instrumentalist method, only the realist can make sense of the regular occurrence of long-term extensions of the observational language and the occasional short-term phenomenon of downplaying successes of old theories.

The Proper Challenge to Sociologists Regarding Non-Falsificationist Behavior

None of this alters the fact that the suggested explanations-cum-justifications of HD evaluation provide an invitation to sociologists of science to offer alternative explanations of the popularity of HD evaluation. To be more precise, sociologists of science have shown convincingly that scientists frequently demonstrate non-falsificationist behavior. However, they have been inclined to look for “social” explanations for that type of behavior, whereas in the view of Zamora Bonilla and myself, straightforward cognitive explanations can be given. Certainly the most important reason is the relativization of the cognitive role of falsification in the process of (even purely observational) truth approximation. This amounts to the difference between HD testing and HD evaluation of theories. Moreover, both methods leave room for many sensible ways in which a *prima facie* counterexample of a favorite theory can be questioned as such. For both claims, see ICR, Section 5.2.3, or SiS, Section 7.3.3. Finally, there is the possibility of the lucky hit nature of successes of a competing theory, referred to above. Hence, in all these cases there are cognitive reasons to expect that non-falsificationist behavior may serve epistemic purposes. To be sure, and this is a major point made by Zamora Bonilla, on average this may well be useful for gaining recognition. Hence, in regard to non-falsificationist behavior, the proper challenge to sociologists is to look for cases that cannot be explained in this convergent way.

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